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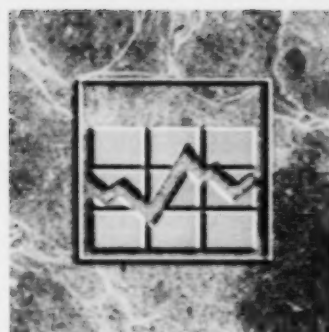
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Dealing with the Seam Problem for the Survey of Labour and Income Dynamics

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**DEALING WITH THE SEAM PROBLEM
FOR THE SURVEY OF
LABOUR AND INCOME DYNAMICS**

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Georges Lemaitre

(prepared under contract for Statistics Canada)

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EXECUTIVE SUMMARY

In recent years a considerable amount of attention has been focused on what is known as the "seam" problem in surveys having a longitudinal design. This refers to the fact that the number of transitions or changes in status observed across the seam when the data for two consecutive reference periods are juxtaposed is considerably larger (sometimes, an order of magnitude larger) than the average number observed in the data reported for a single reference period.

Response errors are the most probable cause of seam biases. For characteristics such as employment status or income reciprocity, errors can be due to omissions or to misplacing events in time. However, standard explanations for response errors based on "forgetting theory" are not supported by the data. Results concerning proxy effects are mixed but generally show no clear association.

A conceptual analysis of possible errors and of results reported in the literature suggests that a tendency for respondents to push back in time, in particular to the beginning of the reference period, changes which occurred somewhat later, may be an important element in producing seam effects.

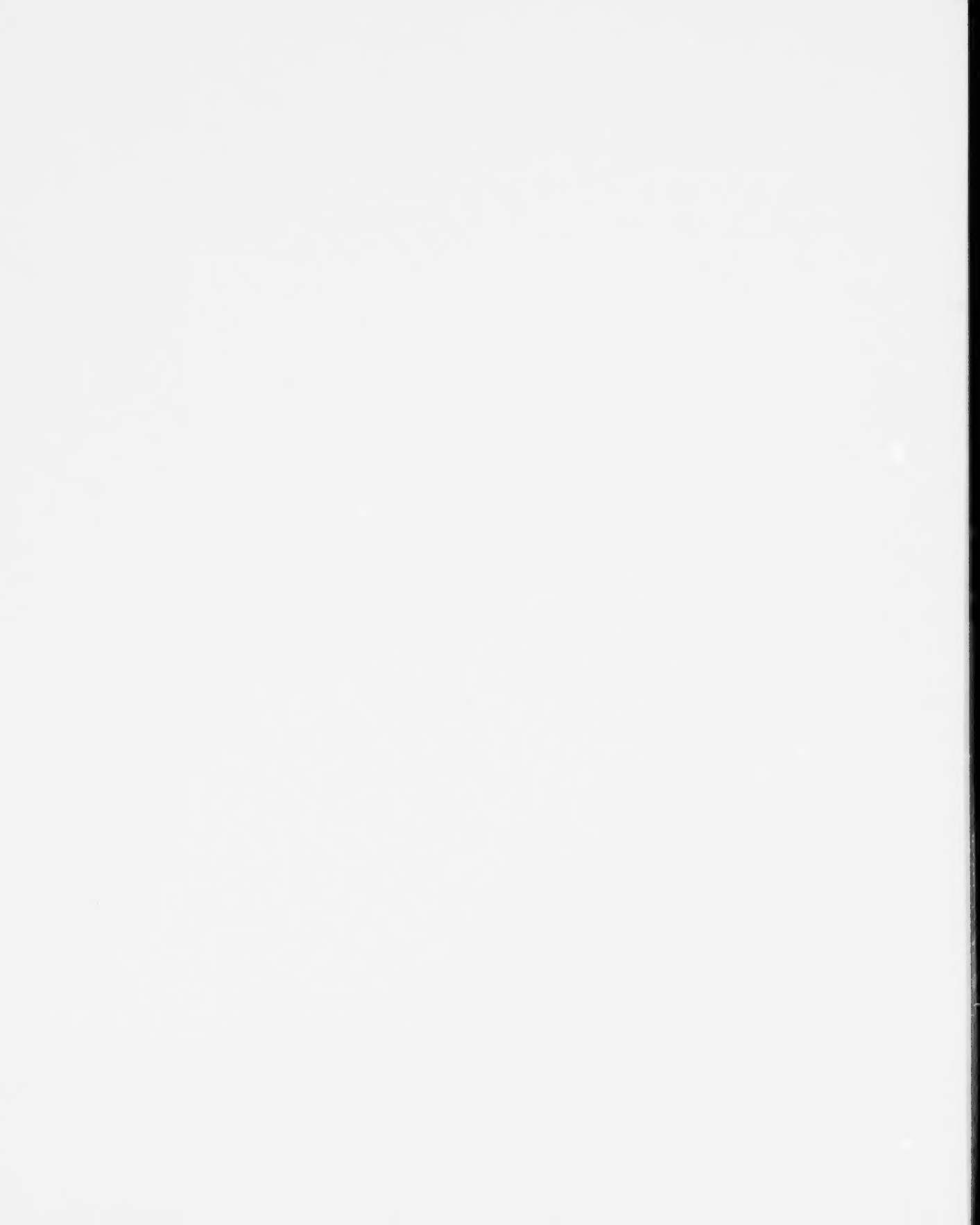
For other characteristics such as wages, unionization, usual hours worked, seam effects would appear as a result of simple response variance.

Dependent interviewing (i.e., feeding back to respondents responses provided on a previous interview) would appear the most appropriate strategy for dealing with seam effects. However, not all feedback techniques will necessarily work. A comparison of two such techniques, one which failed to eliminate seam effects (SIPP) and one which was successful (LMAS) and which has also been successfully tested by SIPP, attempts to identify the key features required.

The paper argues in closing that dependent interviewing should not be viewed as a necessary evil that is required for reducing seam biases but as an integral part of the interview process in longitudinal surveys.

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1. Introduction

In recent years a considerable amount of attention has been focused on what is known as the "seam" problem in surveys having a longitudinal design. This refers to the fact that the number of transitions or changes in status observed across the seam when the data for two consecutive reference periods are juxtaposed is considerably larger (sometimes, an order of magnitude larger) than the average number observed in the data reported for a single reference period.

The high transition rates across the seam do not correspond to any phenomena that are actually observable in the real world and have been attributed to the presence of reporting errors. In particular, an error in reporting on one of two separate occasions on the part of someone whose status has not changed in effect creates a spurious change. The number of these spurious changes can be quite large relative to the true number of changes occurring.

Longitudinal surveys have been relative late-comers in grasping the significance of reporting errors and their impact on the measurement of transitions. The problem has been known among analysts of labour force gross flows for some time (see, for example, NCEUS (1979)), and several estimation methods proposed in order to deal with it (PCGF (1985)).

This paper concerns itself with the seam problem as it is encountered in the context of longitudinal surveys. Although the panel survey structure of labour force surveys does lend itself to longitudinal analysis, these surveys are not, strictly speaking, longitudinal in design.¹ Nonetheless the work carried out on gross flows

¹ A longitudinal survey interviews the same individuals on successive occasions. Labour force survey samples are generally samples of dwellings. If the residents of a dwelling change during its lifetime in the sample, the original residents fall out of the sample and are replaced by the new residents.

has been instrumental in an understanding of the effect of reporting errors on the measurement of transitions.

2. The Seam Effect - an Example

Before reviewing previous work in this area and examining more closely the nature of the problem, let us look at an example.

Figure 1 depicts a cross-classification of persons according to their self-employment status at two successive time-points t and $t+1$, which can be months or years. The number of persons self-employed at times t and $t+1$ is, respectively, $a+b$ and $a+c$, and the net change in the number of persons self-employed from time t to time $t+1$ is $(a+c)-(a+b)=c-b$.

These are the kinds of estimates that are generated from labour

force surveys. With a longitudinal survey, because one has information on the self-employment status of respondents at successive time-points, one can also obtain estimates of the interior cells a , b , c and d (often called the "flows" as opposed to the marginal totals, which are referred to as the "stocks").

In longitudinal surveys as currently conducted, cells b and c , which show the numbers of persons moving out of or into self-employment from one period to the next, tend to be overestimated across the seam (a and d , on the other hand, are

Figure 1.

		$t+1$	
		SE	NSE
t	SE	a	b
	NSE	c	d
		$a+c$	$b+d$

SE: Self-employed
NSE: Not self-employed

underestimated). Note that the net change $c-b$ can still be unbiased, if c and b are overestimated by approximately the same amount.

The actual estimates observed for the above characteristics in Statistics Canada's Labour Market Activity Survey³ are shown in Charts 1.1 and 1.2, with Chart 1.1 giving the estimate of the number of persons self-employed by month for 1988 and 1989, and Chart 1.2 the movements in and out of self-employment for the same period. The seam is the period between the last month covered by the 1988 survey (December) and the first month covered by the 1989 survey (January). In the former case, the information for December 1988 was collected one to three months later, whereas a full year had elapsed when respondents supplied the information for January 1989.

Note the peak in the number of transitions across the seam. This is typical of the pattern one observes for such estimates. The number of transitions is some three to four times larger than what one observes during the rest of the year. There are also relatively large movements out of self-employment observed in July and large movements into self-employment in August and September. However, these likely correspond to real temporary cessations of activity during the summer months. There is no comparable labour market reason for observing large transitions in both directions from December to January.

The Labour Market Activity Survey was an annual longitudinal survey conducted with two separate panels, the first covering 1986 and 1987 and the second, 1988-1990. Interviews were conducted in January through March and information collected on the labour market experience of respondents in the previous calendar year.

3. A Review of Reported Evidence for the Seam Effect

The fact that transitions tended to be higher "between waves" than "within waves"³ was first noted by Czajka (1983) with respect to reporting of income reciprocity in the United States Income Survey Development Program (ISDP), the predecessor to the Survey of Income and Program Participation (SIPP). Moore and Kasprzyk (1984) document significant seam effects for a wide array of data items in the ISDP, including receipt of wage-and-salary earnings, social security, unemployment benefits, employer pensions, aid to families with dependent children (AFDC) and food stamps. Indeed, the only two income sources for which the seam effect was not systematic and significant were closely related - educational benefits and Basic Educational Opportunity Grants. The authors hypothesized that since these sources involved one-time payments at the beginning of the school terms, they were more easily "date-able" than other sources, and that accurate reporting of the single payment could never produce more between-wave than within-wave turnover.

Burkhead and Coder (1985) demonstrated similar effects for SIPP (with its four-month reference period) for earnings, Social Security benefits, unemployment compensation, AFDC, private pensions, food stamps and Medicaid, and this despite the fact that SIPP incorporated a questionnaire procedure for updating the roster of income sources based on the data collected on the previous wave's interview. Hill (1987) reports seam effects in the Panel Study on Income Dynamics (PSID) for unemployment compensation and food stamps. Seam effects are also evident in the example cited earlier on self-employment from the Labour Market Activity Survey and on other characteristics such as job search, the desire

A "wave" is the term used to describe one of the successive interviews of a longitudinal survey. The "between wave"/"within wave" terminology, although somewhat clumsy, is a useful shorthand that has become generally accepted in the literature.

for work and training, which we will examine later in this report. Finally, analyses of gross change in labour force status suggest that the problem exists even for surveys with reference periods as short as one month.

In short then, seam effects would appear to be a general problem with current longitudinal surveys, regardless of differences in design and in the length of the reference period. The next section will deal with the reasons underlying the existence of seam effects.

4. Seam Effects - Why They Occur

We have pointed earlier to response errors as being the likely cause of the seam problem. Let us examine this more closely. Generally, one would not expect respondents to report events which never happened. Much more likely scenarios are that respondents omit reporting certain events entirely or situate them incorrectly in time. This may or may not be accompanied by errors in measures of duration, depending on whether or not misplacing the timing of an event extends or shortens the duration of the prior or succeeding state. In addition, seam effects can occur in the measurement of characteristics such as usual hours worked where the presence/absence of the characteristic or its timing is not at issue. In such cases, it is likely that ordinary response variance can account for seam effects. We consider these only later in this report.

4.1 A Conceptual Analysis of Flows into and out of Self-Employment

To keep the discussion focused, let us return to the example cited earlier concerning self-employment and look first at flows out of self-employment at the seam from December of one year to January of the next. We will look at flows

into self-employment later. To aid the discussion in this section, the reader is referred to Figure 2.

Spurious flows out of self-employment at the seam can occur in two ways:

- a) if persons who were actually not self-employed in both December and January incorrectly report self-employment in December but correctly report it in January;
- b) if persons who were actually self-employed in both December and January report it for December but deny it for January.

We are assuming here that the other possibility, namely that a true transition out of self-employment from December to January has been incorrectly recorded as a transition into self-employment, is highly improbable because it would require a complex and implausible pattern of response errors.

Note that we are stating things here as if the responses were always non-proxy. We will continue stating hypotheses in this way, although one might expect that a non-proxy response on one occasion and a proxy response on the other might contribute to more spurious transitions. However, studies in recent years (Lemaitre (1988), Moore (1990)) have demonstrated that the data-quality advantages of non-proxy response have been somewhat exaggerated. Other studies cited below deal directly with the effect of proxy reporting on the seam problem.

Keeping in mind what was stated earlier about the likely causes of reporting errors, case a) above can occur if persons were self-employed earlier in the previous reference year, left self-employment prior to December, but incorrectly reported their self-employment as extending into December. This is traditionally known as "telescoping", which is the term used to describe the purported tendency of respondents to recall events as having occurred more recently than in fact was the

case (Neter and Waksberg (1965)). The hypothesis of telescoping was advanced to explain the fact that some characteristics tended to be underreported at the beginning of a reference period and overreported at the end. The event that has been telescoped forward here is the transition out of self-employment. The telescoping of the flow out of self-employment would translate into an apparent overreporting of self-employment for December and perhaps for prior months as well.

Case b) can occur if respondents at the time of the second interview completely forgot about the self-employment that was reported at the last interview (a possibility if the self-employment was temporary or of a marginal nature) or if they actually left self-employment later than January but remembered and reported it back to January. This latter case of course is the complete opposite of telescoping, because it suggests that respondents view events as having occurred more distantly than was actually the case. We will refer to it henceforth as "reverse telescoping". Analogous to case a) above, case b) would be observed as an underreporting of self-employment in the early part of the reference period.

Spurious flows into self-employment, on the other hand, could occur, firstly, if persons were actually self-employed both months, but reported no self-employment in December because they remembered the transition into self-employment as having occurred later than December. This seems likely only if interviews are conducted some time after the end of the reference period (in the PSID, for example, interviews are conducted from May through September). The second case is that of persons not self-employed in both December and January, who erroneously report self-employment in January. This could occur if the transition into self-employment occurred later in the year, but is reported as having taken place in January, at the beginning of the reference period. Again reverse telescoping.

To summarize then, false transitions out of self-employment can occur if respondents:

- i) accurately report for January but in reporting for the first reference period,
 - telescope self-employment that ended earlier in the previous reference period into December;
- ii) accurately report for December but in reporting for the second reference period,
 - forget entirely about the self-employment reported on the previous wave;
 - reverse telescope a transition out of self-employment that occurred later in the second reference period back to January.

False transitions into self-employment, on the other hand, will occur if respondents:

- i) accurately report for January but in reporting for the first reference period,
 - telescope a transition into self-employment, that actually occurred in the first reference period, into the following year (applicable only when interviews are conducted some time after December);
- ii) accurately report for December but in reporting for the second reference period,
 - reverse telescope a transition into self-employment that occurred later in the second reference period back to January.

Note that for flows out of self-employment, there is one source of error (forgetting about self-employment) that has no counterpart in the sources of error for flows into self-employment. In addition, for the latter, source i) seems unlikely when interviews are conducted right after the end of the reference period. On the basis of this, one might predict overall more false negatives (i.e., a failure to report self-employment in a given month) than false positives (i.e., reporting self-employment in a given month when it did not occur). As we will see below, for income

recipieny, this is exactly what Marquis, Moore and Huggins (1990) observed in SIPP.

4.2 The Probable Cause - A Hypothesis

Now in longitudinal surveys, the data for the month before the seam is collected with a recall period that is shorter (in annual surveys, substantially shorter) than the data for the month after the seam. Thus if there are in fact recall effects operating, one would expect the data for January in our example to be more misreported than the data for December. This would suggest that reverse telescoping of transitions which occurred later in the second reference period back to the seam may be the principal factor underlying seam effects. For flows out of self-employment, there is the additional possibility that respondents at the second interview may forget entirely about the self-employment that occurred and that was reported on the previous interview.

There are also other factors that might contribute to seam effects. There is the possibility that self-employment was incorrectly recorded in the first place, that is, that it never actually took place. However, as we have noted earlier, one would not normally expect respondents to report incidents that have never occurred. Further possibilities are of a more random nature and include, for example, a misunderstanding of the question by the respondent or of the reply by the interviewer, deliberate misrepresentation by the respondent, and coding, processing or imputation errors.

With this as background, it is instructive to examine what research in the area of the origins and causes of seam effects has uncovered.

4.3 A Review of the Literature

Moore and Kasprzyk (1984) examine the possibility that seam effects may reflect real underlying trends, errors in edit and imputation, or errors in matching records from two interviews. However, they dismiss the possibility of any of these contributing significantly to the seam problem and fall back on the most common explanation given for the effects, namely some form of recall bias.

They state that if respondents retrospectively tend to report less change than actually occurred over a reference period, then this would tend to result in more between-wave than within-wave turnover in two ways, first by reducing the incidence of within-wave transitions and secondly, by shifting the occurrence of transitions back to the between-wave seam. A study by Goudreau, Oberheu and Vaughan (1984) is cited which lends some support to these notions. This study of known AFDC recipients showed that a) persons who failed to report any receipt of AFDC benefits were likely to have received them during only part of the reference period and that b) the most common error in reporting payment amounts was the tendency to report the most recent amount for all [four] months of the reference period when payments had in fact varied. In both cases, the respondent tendency is to report less change within the reference period than actually occurred.

A second explanation for the seam problem examined by Moore and Kasprzyk concerns the effect of proxy reporting. It was hypothesized that spurious changes in status were more likely when different respondents provided the data on the two occasions. However, they found no evidence that self-reporting in consecutive waves eliminated seam effects. In fact, the weight of the evidence suggested that self-reporting on both occasions worsened the seam effect!

In a later study, Weidman (1986) examined the possibility that demographic covariates (age, sex, education, marital status, etc.) or interview status (patterns of proxy/non-proxy reporting) might help explain the higher between-wave turnover observed in SIPP, but found no conclusive evidence of any association.

Hill (1987) examines the seam effect for PSID in the context of one year (1984) for which there was an overlapping seam design. That is, data for January 1984 was collected on both the survey normally covering only the 1983 calendar year and that covering the 1984 calendar year. Thus the December 1983-January 1984 transitions can be examined as reported on one occasion or on two. The analysis dealt with employment status. The respondent reports of employment status were classified into three groups according to the consistency between the 1984 report for January 1984 and that provided in 1985, as follows:

- a) consistent reports;
- b) inconsistencies where the 1985 report showed no change in status since December 1983;
- c) inconsistencies where the 1985 report showed a change in status.

Note that case c) tends to contribute to a seam effect, whereas case b) reduces its impact.

An analysis of possible determinants of inconsistency suggested that blacks, older respondents and persons with shorter tenures tended to contribute to the seam effect, whereas women and persons with higher incomes tended to respond in such a way as to attenuate it. On the other hand, the length of recall (interviews for the PSID are conducted over a six-month period) and the number of transitions that occurred over the 1984 reference year, both of which are normally advanced as measures of the response difficulty facing the respondent, had no discernible effect.

However, the author admits to being unable to provide an adequate causal explanation for the observed results. In analyses of this kind, where there are many potential "explanatory" variables used, some of which may be intercorrelated, the effects may be distributed over a number of variables, so that it is difficult to see which is the significant underlying factor (if any). One study (Lemaitre (1988)) has shown that response errors for employment status tend to be associated with shorter hours and lower tenures (i.e., less stable employment). It is reasonable to suppose that respondents with a record of unstable employment will have a more difficult time reporting it accurately. However, in regression analyses, covariates of unstable employment (such as low education, race, low income) may be picking up some of the effects. It may be that this is what is being reflected in Hill's analysis.

Kalton and Miller (1991) examine the reporting of Social Security amounts in SIPP at a time (January 1984) when there was an actual increase in benefits (of 3.5%). The SIPP sample is made up of four rotation groups which are interviewed in successive months with the four previous months as reference period. Thus data for successive pairs of months are collected on the same wave for three of the four rotations but on different waves for the fourth rotation. This allows a comparison of within- and between-wave reporting for any pair of consecutive months.

The authors report the usual excess of between-wave transitions relative to within-wave reports. They then look more closely at the reporting of the January 1984 increase in benefits for the three rotations which included both December 1983 and January 1984 in their reference periods. They find, first of all, an association between the length of the recall period and the proportion of respondents reporting no change in benefits; that is, the further back the increase occurred relative to the date of the interview, the less likely it was to be reported.

They also model the correct reporting of the 3.5% increase (defined to be any report between 2% and 4.1%) as a function of a set of explanatory variables and found an association between correct reporting and the recency of the reference month, self-reporting, race, and receipt of larger Social Security payments. One question that arises with respect to the non-reporting of the increase in benefits is whether the increase could have been reported at some other time, in particular at the preceding or succeeding seam. Kalton and Miller find a significant tendency for this to occur, especially at the preceding seam. In other words, a significant number of respondents report the amount they are currently receiving as being the amount they have been receiving during the entire reference period. This suggests that part of the seam problem is due to the fact that respondents tend to reverse telescope the timing of events, that is, they perceive events as having occurred earlier than they actually did and in particular, at the beginning of the reference period covered by the survey.

Perhaps the most comprehensive analyses of the seam problem were carried out on SIPP data by Marquis and Moore (1989) and Marquis, Moore and Huggins (1990). They examined reporting for eight programs with participation reported by SIPP households in four states. The survey reports were micro-linked to administrative data for the same programs, permitting a true validation of the survey responses. The results show response problems for practically all programs. In particular, six of the eight programs showed underreporting of participation, with biases ranging from 8 to almost 40%. In addition, transitions were almost always too low off the seam and too high on the seam, relative to the what was observed on the administrative records.

They then examined assumptions from forgetting theory that are traditionally advanced to account for the types of patterns observed. These state that errors are mostly omissions or underreports, that underreporting gets worse as elapsed time

increases, and that recall of recent events is accurate. However, the record check results did not support these assertions. Although underreporting of participation did predominate, all programs were overreported significantly as well. In addition, with the exception of unemployment insurance benefits, there was no significant improvement in reporting for the most recent month relative to four months earlier.

Other results of their study include the following:

- Confusion about program names does occur, but is of minor importance.
- For both proxy and non-proxy respondents, underreporting is more significant than overreporting. The frequency of underreporting is greater for proxy respondents than for non-proxy respondents.
- Interviewer effects are no more important than in other surveys.

Unfortunately, the authors did not consider in detail the possibility that the overreporting observed in a particular month may reflect misplacement or extension of income receipt on the part of respondents who were actually in receipt in other months. Although they do examine the extent of external telescoping (the possibility that income receipt in the previous reference period may be brought forward into the current period), they look at this in a rather limited framework. They expected that, since the interviewer reminds the respondent in subsequent waves of what was reported in the previous wave, it was unlikely that the respondent would report participation in the current wave that had actually occurred in a previous wave. The only exception to this was for wave 1, when nothing was fed back to the interviewer. They therefore expected to find greater overreporting of participation in wave 1, but found no evidence of this.

However, it is not clear why reminding the respondent of what was reported in a previous wave would not result in a respondent's incorrectly extending program participation into at least the early months of the current wave.

4.4 Why They Occur - Summary and Conclusion

In summary then, studies that have attempted to identify the origin of seam effects have not managed to identify a clear underlying cause for the observed results. Results concerning proxy effects are mixed but generally negative. Hypotheses concerning recall have generally not been supported by the data. On the other hand, two studies (Goudreau, Oberheu and Vaughan (1984) and Kalton and Miller (1991)) suggest that respondents tend to underreport change over a reference period and indeed, to reverse telescope the change that did occur back to the beginning of the reference period. This is exactly the phenomenon identified earlier as the most likely principal contributor to the seam problem.

Interestingly, a reverse telescoping of flows out of self-employment back to the seam would, all things being equal, show an apparent underreporting of self-employment in the early part of the reference period. However, this would be counter-balanced by the opposite phenomenon, namely a reverse telescoping of flows into self-employment back to the seam month, which would tend to show an overreporting of self-employment in the early part of the year. In the aggregate, little to no recall effects would be apparent. The presence of both over- and under-reporting and the apparent absence of recall effects in the aggregate are precisely what Marquis, Moore and Huggins (1990) observed in their record-check study.

A further consequence of reverse telescoping of change (either into or out of self-employment) back to the seam month would be an overall underreporting of

change during the reference period, a result again confirmed by the SIPP record check study. In addition, if reverse telescoping of change is more likely for changes that are less recent, there would be more underreporting of change in the early part of the reference period, a result observed by Kalton and Miller (1991).

If indeed the essential feature of the seam effect is the reporting at the seam of change that actually occurred at some other time, then some sort of feeding back to respondents of responses provided on the previous interview (i.e., dependent interviewing) would appear to be in order. The next section deals with this question.

5. Dependent Interviewing - A Solution?

Dependent interviewing has rarely been attempted because of fears that it would tend to bias estimates of change. These fears, however, need to be assessed in a proper context. One might well be reluctant to carry out dependent interviewing in a situation where estimates of change were known to be unbiased. But here the data of interest are longitudinal in nature, and the estimates of transitions across the seam are seriously upward biased. The expected impact of dependent interviewing is precisely in the desired direction. Whether or not dependent interviewing will excessively reduce gross change and whether or not it will induce biases in estimates of net change are questions for which there are as yet no clear answers. We will return to these points later.

Let us observe, first of all, that dependent interviewing can be carried out in different ways, not all of which may be successful or produce the desired result. We will contrast here two such techniques, one which has been used by SIPP since 1983 without success, and one which was implemented successfully by the Labour Market Activity Survey in 1987.

5.1 A Comparison of Two Feedback Techniques

In SIPP, a list of the income sources received during one or more months of the last reference period is read by the interviewer. The correctness of the list is verified and any discrepancies resolved. The respondent is then asked whether income for any of the income sources mentioned for the last reference period was received at any time during the current reference period. Finally, receipt for particular months is determined when the amounts are recorded, beginning with the most recent month (Burkhead and Coder (1985)). As has been documented above, this form of dependent interviewing has been notably unsuccessful in ensuring smooth transitions at the seam.

By contrast, the Labour Market Activity Survey (LMAS) collected information on all jobs that were held during the reference year, including jobs held up to the time of the interview, which is held early in the following year. The purpose of this was to record the jobs of record at the beginning of the following year, and it is this information which is fed back to respondents on the subsequent year's interview. Respondents are then asked to confirm this information, which is then used as the starting point of the current year's interview. A certain proportion of respondents fail to confirm the previously reported jobs, and the LMAS has developed imputation procedures that extend the jobs into the current year in a way that is compatible with the status of these jobs as recorded at the time of the previous year's interview. The aim here is to avoid confrontation with the respondent, while attributing greater weight in processing to the information recorded at the previous year's interview, when the recall period was considerably shorter. The resulting estimates of flows out of employment at the seam showed a complete absence of the usual seam effects (Murray, Michaud et al. (1991)). In addition, estimates of flows out of employment showed considerably less underreporting in the early part

of the year relative to other sources than the first LMAS wave, for which no information had been fed back.

The key feature of the LMAS technique is that the information fed back to respondents effectively bridges the seam, because it is information for the current reference year. Thus respondents, in order to report a change at the seam, must essentially deny a report provided a year earlier under far more favourable recall conditions. Some nonetheless do so (about 2%), but the imputation procedures appear to handle these quite well. Note that denial of jobs reported previously was not found to be associated with proxy response in the LMAS. Recent SIPP tests of this same technique for income reciprocity have also shown no overall seam effect (Marquis (1992)).

The SIPP dependent interviewing technique, on the other hand, reminds respondents of what they reported for the previous reference period, before asking them about reciprocity at any time during the current reference period. On the face of it, this might appear to be a workable technique. However, it does not prevent respondents from reverse telescoping back to the seam any change that may have occurred over the current reference period. Also, respondents are reminded of income sources received during any month of the preceding reference period, not just the final month. Thus there is a possibility that respondents who may not have been in receipt in the final month of the previous reference period, may mistakenly extend receipt into the current period. They may then actually report receipt for the first month of the period, thus contributing to a seam effect.

The results from both surveys are compatible with the hypothesis that reverse telescoping of change back to the seam may well be the principal cause of the seam problem. If this is indeed the case, then dependent interviewing is likely to have a very favourable impact on flow estimates at the seam (as has been demonstrated in

both the LMAS and recent SIPP tests) without adversely affecting estimates of net change or excessively attenuating gross change both at the seam and thereafter. The reason is that reverse telescoping does not imply that respondents have a general tendency to underreport change which dependent interviewing would only exacerbate, but simply that they misplace it in time. Once they are reminded of their state at the beginning of the current reference period (as reported on the previous interview), one would expect the state that would normally have been reported then, if no information had been fed back, should normally be reported somewhat later in the reference period.

5.2 Other Possible Methods of Dealing With Seam Effects

In addition to dependent interviewing, estimation techniques have also been proposed for dealing with the seam problem. However, they require auxiliary information and generally can be applied only one variable at a time, which makes them impractical for regular use. Some of these are documented in Marquis, Moore and Huggins (1990). Of more interest is a study mentioned by the same authors in which Census Bureau staff members in 1989 accompanied SIPP interviewers on interviewing rounds. At appropriate times, they interrupted the interviews to question respondents about the answering process, later summarising the important verbal interactions which they observed.

One of the main conclusions from the summaries is that many respondents adopt a simple heuristic rule of thumb to quickly answer questions about recurring events in the SIPP four-month reference period. Respondents use the simple rule as a substitute for detailed, direct recall and as a substitute for checking their personal records. The authors conclude that response quality could be improved by "teaching" respondents how to provide better information (including the use of records), reorienting interviewers to the importance of response quality, and

ensuring that interviewers explain to respondents the goals of each section of the questionnaire. Although these measures may well improve response quality and perhaps reduce the types of errors that lead to seam effects, they have rather major cost implications and may not be entirely practicable for this reason.

6. Other Results from the Labour Market Activity Survey

We have noted earlier the overestimation at the seam in the LMAS for movements into or out of self-employment and the absence of a seam effect for flows out of employment when information on jobs is fed back to interviewers in a particular way. This section looks at other examples of seam effects from this survey, demonstrates the necessity for doing a "pre-interview" to collect basic information to feed back to respondents on the first wave, and examines characteristics for which seam effects are not so clear.

Charts 2.1, 2.2, 3.1, and 3.2 show monthly stock and flow estimates of job search and desire for work among persons without work the entire year. The question about desire for work was asked only of persons who indicated no job search whatsoever during the reference year. In both cases, the flow estimates show the usual seam effects. However, although the shape of the curve showing the stock estimates for 1988 is roughly comparable to that for 1989 for both characteristics, both also show a sudden drop in the stock estimates from December 1988 to January 1989. Although the recall period for the December estimates is much shorter than for the January estimate, the drop cannot be the result of differential recall in these two months, because the entire curves for 1989 have been shifted down. What could be causing this shift?

The job search and desire for work recorded here are for persons who reported no work during the year. The most plausible explanation for the shift is that there was

a drop in the number of persons recorded in 1989 as without work the entire year. Now information on the jobs of record as of January 1989 were fed back to respondents for the 1989 interview. No such procedure was in place for the 1988 interview, which was the first wave of the survey. It is likely that jobs (probably marginal) which respondents would otherwise have omitted reporting for 1989 (either because they forgot about ever having held them or because they incorrectly recalled no longer having them in January) were actually reported because respondents were reminded that they held them at the beginning of the year. Thus the feeding back of responses on jobs has reduced the number of persons recorded as not having worked at all in 1989 relative to 1988.

The stock shifts also enable us to place a rough lower bound on the number of additional jobs recorded in 1989 as a result of dependent interviewing. It is approximately equal to the difference between the sum of the observed stock estimates for the two characteristics from December to January and the sum of the true net change for the two characteristics over the same period. The estimates are summed for the two characteristics because they cover different groups of respondents, that is, persons were asked about their desire for work only if they indicated no job search whatsoever during the year. We do not actually know what the true net change from December to January was, but to obtain a rough estimate, we can assign the largest net change that occurred over the 1988-1989 period (excluding of course the December to January change) to December-January. The estimate of additional jobs comes to a little under one hundred thousand. Note that this is also the amount that this group would have contributed in the way of spurious flows out of employment had their jobs not been identified in 1989.

These particular examples illustrate the necessity to feed back information on the first wave, as well as successive waves, if stock shifts of the kind recorded above for the LMAS are to be avoided.

Charts 4.1, 4.2, 5.1, and 5.2 show stock and flow estimates over 1988-1989 for full-time school attendance and full- or part-time job training. School attendance shows a blip at the seam, but this is obviously overshadowed by the real flows into and out of full-time school attendance one observes during the year. In addition, although the flows at the seam are substantially higher than those for adjacent months, a good portion of these may in fact be real, because December and January are natural transition months for school attendance. In cases like this, where there are clear institutionalized memory cues to aid the respondent in timing events, the number of spurious flows may in fact be quite small.

Likewise, training flows show blips at the seam, but it is not immediately clear whether these are spurious or not, because comparably large flows exist at other times of the year. However, the data do provide some clues. One would expect that the large flows into training observed from August to September would be followed some time later by large flows out of training, when the training ends. The training periods may be of variable duration, which would mean staggered outflows and no clearly visible peak. Or the training periods may be largely concurrent, with a sudden peak in the outflows. In our case, one does observe a peak in the outflows which happens to occur right at the seam, implying a training period of September through December. Likewise, the peak in training inflows observed at the seam is not incompatible with the training outflows one observes in subsequent months.

Nonetheless, the assumption that the flows observed at the seam are real is a risky one, and one would be ill-advised to forego dependent interviewing in this case without independent corroborating information.

7. Dependent Interviewing - How Much is Enough?

The examples concerning school attendance and training cited above raise the question of how extensive dependent interviewing needs to be in a longitudinal survey in order to ensure reliable estimates. Must one feed back information on all respondent characteristics as they were observed on previous interviews?

7.1 When to Feed Back and When not to

School attendance is one example of a characteristic where it may not be necessary to do so, but one is at pains to identify other characteristics where a comparable "natural" framework for timing events exists. The case of training, for example, is less clear.

Certain characteristics (e.g., whether or not a respondent works at a unionized job or is covered by a pension plan) can probably be assumed not to change, unless the worker changes employer or perhaps gets promoted into the ranks of management. Thus one need not even ask such questions in subsequent interviews unless there is a change in status that might result in a change in the characteristic. Indeed one is asking for trouble by re-asking such questions without dependent interviewing, especially if there is no change in status that would warrant it. Note that seam effects here, as in other examples cited below, would likely result not from forgetting or misplacing events in time, but from ordinary response variance.

Tabulations of wage change for wages received at the same job but recorded independently on two interviews one year apart suggests that it would be advisable to feed back information on wages (Bootby (1991)). The study showed implausible numbers of workers had reported reductions in their wage rates over the year. In this case, seam effects are likely to be the result of an inaccurate reporting of wages and may in some cases reflect the impact of proxy reporting.

Other characteristics such as usual hours are known to be poorly reported and are likely candidates for strong seam effects. One study (Lemaître (1988)) has shown that even with only one intervening week between reports, approximately one third of respondents reported different usual hours for the same reference week. The discrepancy rate scarcely differed even when the hours were self-reported on both occasions. In this case, one could opt for assuming no changes in hours for the same job, and simply confirm whether the job was still full-time or part-time.

One criterion in deciding whether to feed back information on a particular characteristic might be whether or not the characteristic in question is likely to be the object of transition analyses. This is a critical point, because Hill and Hill (1986) found that in a proportional hazards model of transitions into employment using SIPP data, whether or not the week of the transition was a seam week was the single most important predictor of a transition. Unfortunately, it requires substantial powers of pre-cognition to determine which variables will never be used in transition analyses.

For some characteristics, one option might be flagging changes to the interviewer (which is possible in a CAPI framework) and having them confirmed with the respondent. However, this strategy may cause problems for the interviewer if there are many changes being recorded and the respondent is repeatedly having to confirm a change that the respondent will know can only be identified if the

interviewer refers to what was said on the previous interview. It is not likely that the respondent would appreciate the perhaps unintended implication, if many such confirmations are forthcoming, that the responses being provided are somewhat less than ideal.

7.2 Feedback as an Integral Part of the Interview

In short then, there does not appear to be any easy answer to the question of how much information should be fed back to respondents. The one certainty that exists is that if one does not feed back, seam effects will undoubtedly occur. However, the feeding back of information need not necessarily be overly intrusive or involve excessive response burden, depending on how it is done. CAPI allows for the possibility of tailoring questions to particular situations. For example, one need not ask the respondent to confirm what was reported a year earlier (as was done for jobs in the LMAS), but simply assume that the previous report was accurate and carry on the interview from there. For example, in the case of jobs, one might ask "Are you still working for...?" This is comparable to what is currently done in the Labour Force Survey for industry and occupation. This might be followed by "Did you have any promotions with this employer?" If the answer to the first of these is "yes" and to the second is "no", there are many questions that need not even be asked again or even confirmed, such as industry, occupation, unionization, pension plan coverage, etc.

In addition, the feeding back process should not be viewed as a necessary evil that must be carried out merely to avoid seam effects, but as an integral part of the interview process. One way of visualizing a longitudinal survey is to see it as a vehicle for measuring change. With this perspective, the benchmark interview essentially establishes the situation of the respondent at the time of the interview. The role of subsequent interviews is then to identify what change occurs thereafter.

The feeding back of information is then an operational tool to aid the respondent in accurately reporting change and in particular, in ensuring that the change reported across two interviews looks comparable to the change reported over a single interview.

8. Conclusion

The reporting of excessive change at the boundary between two interviews is a problem that all longitudinal surveys implemented to date have been subject to. Now with the benefit of a certain hindsight and with our current knowledge of response errors, it does seem in retrospect hopelessly naive of survey-takers to have expected individual data to be "smooth" when collected on two occasions one year apart. The mystery is in understanding why the problem was not identified earlier, especially since some longitudinal surveys (e.g., PSID and the US National Longitudinal Surveys) have existed for over twenty years. The answer is probably that with the scarcity of data on social issues at the time such surveys were implemented, attention was focused far more on the results of measurement than on its process.

The focus on the seam problem in recent years has drawn attention to the fact that longitudinal surveys must be longitudinal not only in the sense of interviewing the same respondents over time, but also in the sense of collecting data that are consistent over time. In practice, it would appear that the only reliable way of doing this is to feed back to respondents information provided on a previous occasion. There has always been a fear that this kind of procedure would result in reduced estimates of change; however, the consequences of not doing would appear to be even more serious.

The Labour Market Activity Survey has been a notable pioneer in the area of dependent interviewing. Indeed its technique for feeding back information on jobs is the only documented case of success in attenuating, indeed eliminating, seam effects for a particular characteristic. However, since seam effects appear to be widespread and involve most characteristics, a more general strategy is called for. It seems clear that success in this area will require a general style of dependent interviewing as an integral part of the interview process.

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